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THE PROGRESS OF PHYSICS.

The Progress of Physics during Thirty-three Years (1875-1908). Four lectures delivered to the University of Calcutta during March, 1908. By Arthur Schuster, F.R.S. Pp. x+164. (Cambridge: University Press, 1911.) Price 3s. 6d. net.

IN the year 1908 Prof. Schuster during his visit to India gave to the University of Calcutta four lectures, which he has now incorporated into a book, dealing with the progress which had been made in his subject during his own active lifetime, namely from the year 1875 onward.

The scope of the book he thus explains:—

"It is my aim to trace the changes in our point of view, rather than to give an historical account of the sequence of the discoveries which make this period memorable. . . . I prefer to be frankly subjective, and warn you beforehand that my account will be fragmentary, and to a great extent reminiscent of those aspects which have come under my own personal view."

In his preliminary survey of the state of the science before 1875 he contrasts the endeavour at that time prevalent to get a clear idea of the mechanism underlying the various processes, with the "blinkers" which are in some quarters willingly accepted now, not only as inevitable but as praiseworthy instruments conducive to a rational philosophy. The one idea of Energy is now sometimes upheld as dominating the field, and rendering unnecessary all more picturesque inquiry into forces and motions and coordinates in detail.

It is true that ignorance of detail may often be a dominant feature in both methods of dealing with phenomena; but the modern treatment, as he says—"glorifies our ignorance, while the other accepted it as a regrettable necessity." . . . The "vagueness which used to be recognised as our great enemy is now being enshrined as an idol to be worshipped."

At the beginning of the period dealt with by the author this tendency hardly existed:—

"The tendency to hide ignorance under the cover of a mathematical formula had already appeared, but was not openly avowed; hence students were still taught to form definite ideas of the processes of nature."

So far an advantage seems to him to lie with the past; but then, on the other hand, he recollects a less excellent tendency which then existed to regard the scope of physics as already fairly complete, with all the main avenues explored, and only side tracks and subsidiary paths awaiting the would-be discoverer—whose aim sometimes was to get hold of some neglected residual effect, by employing extremely sensitive instruments and applying power enough to bring it within the compass of observation; and whose other and highly estimable aim was to measure with careful accuracy effects already known.

This condition of things Prof. Schuster thus describes:—

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"In many cases the student was led to believe that the main facts of nature were all known, that the chances of any great discovery being made by experiment were vanishingly small, and that therefore the experimentalist's work consisted in deciding between rival theories, or in finding some small residual effect which might add a more or less important detail to the theory."

It is perhaps singular that such an outlook should have so closely preceded an outburst of novelties such as have of late years aroused the enthusiasm of the worker, as well as the occasional derision of the sceptical critic.

"Looking back now on this period, when Röntgen rays and radio-activity were undreamt of, we may well learn to be cautious in our own predictions of the future."

Nevertheless, some of the discoveries were made in what may be called the old-fashioned way, one example being of the first magnitude:—

"A typical example of a great discovery brought about by the method indicated by Maxwell is furnished by Rayleigh's measurements which culminated in the discovery of argon, through a research undertaken to determine with accuracy the density of gases. . . . It is nevertheless indisputable that the greatest discoveries, both formerly and in recent years, have not originated in the hunt of residuals."

No; they have sometimes been begun accidentally, but they have been followed up by those who have been guided by a clue of theory; and some of the most splendid have been made by mathematical physicists who were clearly conscious all the time of what their aim was and what was the meaning of the phenomena they observed.

"Altogether I am doubtful," says Prof. Schuster, "whether any great discovery has ever been made by anyone who has only aimed at recording a number of facts."

Those who condemn theory, and who affect to disbelieve the explanation which physicists give of recently observed phenomena, would do well perhaps to realise that ultra-scepticism has often been a drag on scientific progress, as well as occasionally a safeguard and a help.

As an example tending in this cautionary direction we may quote from the author as follows:—

"The frame of mind with which the academic physicist [or, we might add, the academic chemist] looked upon investigations of the passage of electricity through gases, might be made the subject of instructive comment. The facts, so far as they had been ascertained, did not fit in with recognised views: hence they were ignored, and students were warned off the subject. There was a feeling that perhaps in a century or so the question might be attacked, but that in the meantime it had better be left to be played with by cranks and visionaries. No criticism was more frequent, at that time, than that of characterising as premature any new idea or fresh line of investigation in this direction; as if any advance of science has ever been made which was not premature a fortnight before it was made."

The extreme usefulness of a working hypothesis or theoretical clue can scarcely be emphasised better than by the following:—

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"If we look back upon these experiments now, they may be used to point the moral that experiments conducted in what is sometimes considered to be the true philosophic spirit, where the investigator without any preconceived theories or notions simply wishes to classify facts, seldom lead to any valuable results.

"Progress began when the subject was attacked with some definite object in view, either some theory however crude which had to be supported, or some numerical connection which had to be investigated."

It has been one of the features of Sir J. J. Thomson's work, for instance, that theory and experiment have gone hand in hand and have been published together. The reviewer has heard this feature objected to, but it is more than justified. The publication of the great discovery which is being here specially referred to is thus mentioned by Prof. Schuster:—

"The lecture in which the above experiments were described, was delivered to the British Association [at Dover] on the occasion of the visit of members of the French Association, which met concurrently at Boulogne.¹ It at once carried conviction, and, though to those who had followed the gradual development of the subject it only rendered more certain what previous experiments had already plainly indicated, the scientific world seemed suddenly to awake to the fact that their fundamental conceptions had been revolutionised. A new era of science begins at this point."

Prof. Schuster had himself made experiments which would have received their quantitative explanation had the idea of a separate atom of electricity detached from matter been at that time fairly conceivable. But, as he says—in full accordance with the attitude of most others at the time—

"The separate existence of a detached atom of electricity never occurred to me as possible, and if it had, and I had openly expressed such heterodox opinions, I should hardly have been considered a serious physicist, for the limits to allowable heterodoxy in science are soon reached."

There were not wanting critics to emphasise the heterodoxy of the idea, even when the discovery had been made; and chemical scepticism concerning *ions* exerted a retarding effect in Britain:—

"This ionic theory of gas discharges, while ignored in England, made good progress abroad; Arrhenius adopted it, as well as Elster and Geitel."

But it is in connection with the subject of terrestrial magnetism and its explanation that the author makes the most biting comment:—

"There can only be one solution of the problem, and if we can explain any magnetic effect on the earth's surface by outside forces, it follows that it cannot at the same time be explained by internal forces. This remark disposes of a good deal of the criticism lavished on pioneer attempts to open out this region of science. Though this criticism is often confined to a judicious shrugging of the shoulders, it stops scientific progress more effectually than active opposition, and is apt to become a constitutional habit with those who give way to its undoubted temptations."

Of course, caution is necessary; but on the whole, says the author—

¹ The title originally was "On the Existence of Masses Smaller than the Atoms," but was changed when the paper was published in the *Philosophical Magazine*, xlviii., p. 565 (1899), to "On the Masses of Ions in Gases at Low Pressure."

"The state of plasticity and flux—a healthy state, in my opinion—in which scientific thought of the present age adapts itself to almost any novelty, is illustrated by the complacency with which the most cherished tenets of our fathers are being abandoned."

For instance,

"Nowadays the student finds little to disturb him, perhaps too little, in the idea that mass changes with velocity; and he does not always realise the full meaning of the consequences which are involved."

This part of the treatise is so important, and the usefulness of theory as a guide to discovery is so vital, that we must dwell on the author's treatment of the more philosophical aspect of physics with special interest.

First of all he admits that we must allow something for intuition:—

"When we consider two rival theories in any branch of human knowledge, we are sometimes drawn towards one or towards the other, not by any process of reasoning, but by an instinctive feeling which may be so strong that we unhesitatingly reject one of the alternatives. . . . The strongest of our scientific 'instincts' is our ultimate belief in the simplicity of nature. If both light and electrical attractions are transmitted through a medium, it would revolt our feelings—that is to say, our non-reasoning faculties—to assume, without strong evidence to the contrary, that two different media exist for the two manifestations."

But it is not to be supposed that the intuition is always right. The example which the author takes—in order to illustrate the useful consequences of even an erroneous theory—is not one with which the reviewer sympathises, for he is one of those who desire dogmatically to deny the possibility of physical action except through a medium; whereas Prof. Schuster says:—

"To me it seems that the dogmatical denial of action at a distance is a survival of the ancient anthropomorphic explanation of natural phenomena."

He admits, however, with cordiality that the search for the medium has resulted in many discoveries, and, like many other theoretical clues—whether correct or not—has been justified by results. From this, he says:—

"Two lessons may here be learned. One is, that the temporary success of a doctrine does not necessarily justify the grounds of its foundation; and the other, that progress in science is more often achieved by a definite hypothesis, which may be followed up and tested, than by a wider and perhaps more philosophic doctrine, which cannot be disproved because it does not endeavour to go deeper than the mere descriptive classification of phenomena."

In the same spirit as that in which he maintains the possibility or conceivability of physical action at a distance, the author is not frightened away from any hypothesis because of its excessive strain upon ordinary common sense. For instance, he is able to contemplate the view that an atom or an electron may be a source or a sink of fluid, continually appearing out of nothing at some points and continually going out of existence at others.

"The universe must have begun by a process which lies outside physical laws, and it seems to me no

easier to grasp the conception of a creation which took place at one single time than a creation which continues throughout all ages. Indeed, if we come to think of it, the continuance of a physical law like that of gravitation is as much a miracle as the continuous uniform creation of matter would be."

In this, as in a few other matters, the reviewer is unable to follow him completely.

Coming to another subject, the lecturer has some wise things to say regarding laboratory teaching in physics; some of which—like much else of school teaching—is calculated rather to promote dislike and dullness than to arouse any enthusiasm for science; and whatever may be the merit of college courses in physics, he cannot refrain from noticing how little of that sort of training was available for, or was needed by, the great discoverers.

"The cardinal fact to bear in mind is that previous to 1870, when laboratory instruction, if given at all, was sporadic, the experimental skill of investigators was as great as it is now. We need only mention the names of Faraday, Joule, Helmholtz, and Regnault, in support of that contention. The conclusion is irresistible that an intelligent student, possessing a sufficient theoretical knowledge, is capable of starting research work in physics without previous special training. It is not for him that complicated laboratory courses have been designed, but for the ordinary student."

It is very desirable in all cases that the stimulation of interest shall not be subordinate to the acquisition of mere technical skill. Without enthusiasm, the mere ability to measure all sorts of uninteresting quantities is of comparatively little use.

"The future investigator will no doubt ultimately save time if at an early stage he acquires a certain technical skill and becomes acquainted with physical methods, but otherwise the efforts of the teacher should be directed to stimulating his scientific activity rather than sending him through all manipulations which he might possibly have to perform."

That the author is a great authority in the subjects of terrestrial magnetism and atmospheric electricity is well known, and in his last lecture he enters into these rather fully, compared with his rapid survey of other subjects. Hence these portions have a value of their own, and a few *obiter dicta* may be extracted.

Concerning the view which has been held about the annual and diurnal variations in terrestrial magnetism, he says:—

"A better agreement may be obtained by making the plausible hypothesis that the electric conductivity of the higher regions of the atmosphere is due to solar radiation, and is therefore greater in summer than in winter, and also greater in daytime than at night."

On the subject of atmospheric electricity:—

"The earth as a whole is charged negatively, and on the average we find that there must be nearly a million electrons on every centimetre of its surface. . . . Lenard, in an important research, has shown that a drop of water as it falls never reaches a velocity greater than 8 metres per second, however large the drop, while drops having a diameter of 1.5 millimetres fall with a velocity of about half that amount. . . . The larger drops break up in the air, and doing so become positively electrified, according

to Dr. Simpson. If the ascending current spreads out laterally near the top of the cloud, the vertical velocity is diminished, the drops will grow and fall, but only to break up and be carried upwards again. A quantity of electricity large enough to account for the lightning discharge can thus accumulate in a cloud."

We have thus taken a rapid survey of this interesting book and given a sample of its contents.

The only fault we have to find with it is a minor one, that ought not however to pass unnoticed,—the punctuation is of a systematically irritating kind. In the extracts here made it has in most places been corrected, so that the defect will not be noticed in them, but before concluding we must quote without correcting. There is evidently some one among the printers or readers for the press who has a theory about commas; one rule apparently being that a comma must always precede a relative pronoun.

To illustrate the nonsense which is often thus made, it will suffice to quote a few sentences at random:—

P. 158: "To put the result in a form, which is readily appreciated, I will compare different molecules," &c.

P. 98: "Energy cannot be expressed entirely by quantities, which merely depend on change of position (kinematical factors), but must involve," &c.

P. 139: "Nothing has been said as yet on the explanation of the secular variation of terrestrial magnetism, and in our ignorance of the causes, which make the earth behave like a magnet, it is perhaps wisest to put the question aside for the present."

P. 130: "The discussion of the problem [of diurnal variation of terrestrial magnetism] . . . shewed that there is a substantial remnant which comes to us from the inside of the earth, and this is, as it should be, because the earth being a conductor, any change of the magnetic forces must give rise to induced internal currents."

The main part of the cause of diurnal variation, however, lies above the surface of the earth, and is due to electric currents in the upper atmosphere,—says the author; who thereby seems to contravene a more general pronouncement, quoted above, as to the uniqueness of a given explanation. It is probably the more general pronouncement that he would wish to modify.

The immediately preceding extract is of interest for its own sake, and not merely as an illustration of seriocomic punctuation. So is the following:—

P. 156: "It is always satisfactory when we find that different lines of reasoning lead to the same result, and at present there is hardly one, in the domain of Geo-Physics which stands on so firm a basis as that giving to the earth an extremely high rigidity."

In the selected quotations we have done less than justice to the author's treatment of the many problems which have coincided with his active life, and to the solution of which he has so often contributed; but the book is accessible and very readable. We may be grateful to the University of Calcutta for having stimulated the production of these lectures, and to the author for writing them out and publishing them. They constitute another link in the chain which is binding together East and West.

OLIVER LODGE.